number and implied outlay. But it is a fact—for which we can most positively vouch—that the good work done at the present time in England with the microscope has fallen very far behind its original proportion to the number of instruments in use. In fact we have in the history of English research with the microscope, a typical case of the breaking down of dilettantism. Nearly all the discoveries easy to hand, which could be made by half-an-hour's pleasant peeping through a good microscope have been made, and the numerous observers formerly urged on by success have now become reduced to mere collectors of diatoms and mounters of fleas. On the other hand, in Germany, an efficient microscope was produced somewhat later than in this country, on a different system and at a far less cost. It was not taken up by men of leisure and means, nor were the makers tempted to produce the most elaborate and costly mechanical contrivances for no practical end but their own profit. The microscope in Germany fell into the hands of the professed students of science in the universities, and in consequence the art of applying this instrument to the study of structure has steadily advanced in that country, until not only are all the important observations which are made with the microscope made in German laboratories, or by those who have studied in them; but the whole art or "technic" of microscopy has become a German one. When we find that it is necessary, for some purposes, to watch a single cell for twelve hours or more consecutively, and that three or four months' daily labour is not considered too much to devote to advancing one small step in the knowledge of such a matter as the nerve branches which go to the glands in a frog's tongue, we are not surprised that the *dilettanti* are no longer of service in the progress of researches with the microscope. Thorough and single-purposed men are required for such work; in short, "men of science," supported by special institutions.

It is then with very mixed feelings that we contemplate Mr. Martin's book on Microscopic Mounting. It is of about as much use for scientific purposes as would be an alchemist's guide-book in a modern chemical laboratory. Foreigners who do not know of the immense number of microscopes annually sold in this country as toys, will wonder who on earth can make any use of it. Microscopic mounting is a proceeding which bears about the same relation to the genuine microscopic study of an organism, as enclosing a "subject" in its coffin does to human anatomy. The object which is aimed at by the modern student of histology is not to "mount" a pretty specimen, but to apply such staining, clarifying, coagulating reagents, and such methods of disruption, slicing, hardening, or softening, as will enable him to discriminate, describe, and draw structure; or, it may be, so to arrange the somewhat restrictive conditions of microscopical examination as may enable him to observe with the highest powers the tissues of organisms in their *living* state. He does not care a dump for "mounting," and if he has in the course of his work prepared some hundred or two preparations of an organ or organism which he is investigating, the preparations are usually thrown away when the problem under investigation is solved, or lie for years undisturbed in the catacombs of some cabinet. Mr. Martin is of no assistance whatever in all that relates to the use of reagents or adjuncts to observation with the microscope. He is evidently quite ignorant of German; he gives a number of receipts taken from a variety of antiquated English sources; others he takes without acknowledgment from the "Quarterly Journal of Microscopical Science;" see, for example, the paragraph on staining tissues, p. 144, which can only serve to mislead. Some, of course, amongst Mr. Martin's instructions are correct; but we feel convinced that he has not made trial of even the few methods which he has imperfectly described; and we decidedly object to his assuming the office of a guide, when it is but a case of the blind leading the blind.

Still, Mr. Martin's book may be looked at as an interesting specimen of the phase which English *dilettante* microscopy has now reached. Had the author been a little more careful in confining himself to what he really understood as a diligent mounter of microscopic objects, his volume would have had a genuine antiquarian interest, and would have perhaps been useful; indeed, it may even now in some ways be so to those who take a view of the microscope similar to his own.

It would hardly have been worth while expending so many words on a book in itself of so little significance, had not there been two conditions in existence which, if passed by without remark, may have some consequences which had better be avoided. Firstly, the number of persons, medical students and others, now entering on the serious study of histology, and the use of the microscope as an instrument of scientific research, is increasing. They want books to supplement the efforts of teachers in putting them into the way of working and explaining to them—if we may so say—the most useful "dodges.' Secondly, there are other books besides Mr. Martin's which profess to give instruction on microscopic manipulation, and are all *worthless*. The students may, in their innocence, be led to purchase such books. We wish them to avoid making this mistake. There is no satisfactory book in English on the subject. Beale's is the best, but too large, and not sufficiently complete excepting as to his own methods. Frey's book, which exists in both a French and a German form, is the best foreign guide to microscopic *Technik*.

OUR BOOK SHELF

Magnetism and Deviation of the Compass. For the use of Students in Navigation and Science Schools. By John Merrifield, LL.D., F.R.A.S. (London: Longmans and Co.)

THE Admiralty Manual of Compass Deviation with its auxiliary Elemenatry Manual would, after some lapse of time, appear to have been turned to good account by teachers of navigation. The small volume under review is one of a plentiful supply which appears to be now issuing from the press on a subject of vast importance to seamen of the present generation; and as such is deserving of notice.

It contains useful information of an elementary kind, although this information is not presented either in the practical form or exact and mathematical arrangement of the two manuals we have noticed as the fountain heads of the science of magnetism as applied to iron ships and their compasses.

It is to be regretted that in a subject where clearness and precision are essentials to teaching, Dr. Merrifield

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should have written occasionally with looseness and inaccuracy; for example, at p. 52 we find "Vertical iron at the same place will produce the same deviation in whatever direction the ship's head may be, because a vertical line makes always the same angle at the same place with the line of force." There is confusion of cause and effect here—the deviation is not the same in whatever direction the ship's head may be, although the force remains constant or nearly so—and these are important features for the student to realise.

Notwithstanding, however, these shortcomings, Dr. Merrifield's "small manual" may in the main be received as an orthodox production, and will tend to divert the attention of his students from some singular statements and conceptions relating to the deviation of the compass on board ship and its compensation, to be found at pp. 20-23 of his Navigation and Nautical Astronomy published conjointly with H. Evers in 1868.

We hope to see manuals of this smaller class emanating from seamen, who, possessed of the knowledge of the exact requirements in the practice of their calling, can extract from original sources, and simplify the main points in the various divisions of the subject.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Solar Spectroscopic Observations

I HAVE just seen Herschel's letter to you in NATURE of October 3, and am induced to address you, lest his remarks on Indian Climate should make difficulties in respect to the British Association's proposal for a physical observatory for solar observation.

I do not think Captain Herschel has been in England since the Eclipse of 1868, and consequently he cannot speak from experience of the English climate. It is beyond all doubt that from some cause (commonly believed to be haze) there are really very few days when the protuberances can be studied in England. Herschel's letter contains evidence that that cause, whatever it be, is absent in India. We need not go further; we have the two facts (1) that it is impossible in England, with great telescopic power and great dispersion, to see the prominence lines regularly; (2) that it is possible to do so in India, even in the plains at a bad season. The result seems to be that solar investigations should be pursued in India systematically.

I may add that the facility of seeing these things, and the probability that spectroscopic inquiries into the celestial bodies would have been more successful in the earlier stages had there then been any observers in India, are to my mind a strong argument for now establishing in India, and on a permanent footing, an observatory, whose speciality shall be "Researches on the Physics of the Sun and Planets," whether by the spectroscope, or vision, or photography. When the proposal of the British Association is made formally to the Government by the Council, it will, I trust, take this form. There is no such institution now in the British dominions, and when one is established, it should be in the Tropics somewhere, and as systematically devoted to physical researches as Greenwich has been to the moon.

The Director might, probably would, as at Greenwich, in time attach other investigations; but these researches should be his primal object; and, if they were made so, there is no reason why work as standard and as useful should not result.

October 4 J. F. TENNANT

Consciousness and Volition

THE question raised by Mr. Bennett in last week's NATURE is of great importance, and of no small difficulty. During a visit to the late Sir William Hamilton in 1855, this subject came up for discussion. Sir William was then engaged on his edition of the works of Dugald Stewart. I called his special attention to Stewart's doctrine regarding the operation of Will in acts which are usually ascribed to Habit. Stewart asserts that all habitual actions are really voluntary. As he had no acquaintance with the modern doctrine of "latent mental modifications," he would naturally take for granted that there can be neither a volition nor any other act of mind without accompanying consciousness. He accounted for the non-remembrance of that consciousness by the extreme rapidity of the volitional action. (Collected Works, vol. ii. chap. ii.) To this Sir William objected. He went on to show that in many cases an act, or even a long series of acts, originally voluntary, have ceased to be so. The habit or habitude, which is a mental tendency, though not a power, generated by custom, supplies the place of volition. In illustration, he referred particularly to the well-known fact that in India soldiers will march long distances when they are asleep. Now, it seems to me that this decides Mr. Bennett's question. Here we have regulated action, determined, not by volition, but by habit. Sir William, however, failed to meet all my difficulties, because, as I afterwards saw, of his unsatisfactory theory of causation. He so frequently confounded conditions with causes. With him a cause denoted anything without which an effect could not be; hence his doctrine of con-causes, a plurality of causes for each effect. Thus, when I will to move my hand, and the movement follows, Sir William would call the volition *one* cause of the movement, whereas it is merely a condition. It is remarkable that he should fall into this error since he rejected Biran's doctrine regarding the efficiency of volition. In a subsequent conversation we discussed the points of similarity and the points of difference between Habits and Instincts. It would, however, be trespassing on your space to give the details.

I will only add that an "unconscious volition" in the sense intended by Mr. Bennett is not possible. Dr. Carpenter's expresion "unconscious cerebration" I regard as unfortunate, since it appears to rest on the assumption of the essential identity of mental, vital, and physical powers. Mr. Bennett will find many valuable observations on the nature and conditions of habitual acts in Hamilton's lectures on "Metaphysics," Morell's "Outlines of Mental Philosophy" (published in 1862), and Sir Henry Holland's "Mental Physiology."

JOHN MOORE

Stamford House, Sale, near Manchester, Oct. 14

The Solar Spectrum

UPON reading the communication from Capt. Herschel in your number for October 3, upon the solar spectrum, I seem to remember a letter from a correspondent being published some year or more ago in your pages, in which it was stated the writer had seen the bright lines near the sun's limb with one of Browning's direct vision prism spectroscopes, the instrument being placed on the back of a swing looking-glass as a stand. The dispersion of this instrument would be probably rather more than that of one angular prism. I am bound to say that I have been unable myself, up to the present time, to do more than see a bright line near D, superposed on the solar spectrum, with such an instrument as, however, for other purposes, is a most convenient and sufficiently powerful one.

To my own I have added three small wedge-shaped pointers cut out of thin brass, and fixed in the eye-piece, while the slit-plate, and consequently the spectrum itself, is drawn across the field by a micrometer screw having a range of about 600 divisions between its starting point near A and G at the other end of the spectrum. In this way a bright line (say the auroral one) may be brought upon one of the pointers, and its distance from D, in a salted spirit-lamp flame, at once accurately measured and mapped down; and if the pointer is again brought on the line after the observation, it may be verified by the position of the pointer next day upon the solar spectrum itself.

It occurred to me the other evening to try the effect of a V-shaped slit, and it seems to me to have some advantages. The lower ends of bright lines being brought to a fine point, are more easily positioned on a scale or pointer, while the tapering of faint or nebulous lines or bands enables their relative intensity to be more easily compared. My experiment was a rough one, and with a home-made slit; but for working purposes, by a sliding plate a slit might be contrived of a V shape, whose width and length would be altered at pleasure; and it would, I think, for some observations give good results.

Guildford, Oct. 7 J. RAND CAPRON

A Day Aurora

A CONTROVERSY was carried on in your columns about a year ago as to the possibility of an aurora being seen during the day time. A recent communication from Padre Secchi to the French

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